Journal of the
Experimental Analysis of Behavior
JOURNAL OF THE EXPERIMENTAL ANALYSIS OF BEHAVIOR 2014, **101,** 161–164 NUMBER 1 (JANUARY)

"ASSOCIATIVE CONCEPT LEARNING IN ANIMALS" BY ZENTALL, WASSERMAN, AND URCUIOLI: A COMMENTARY

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This opportunity to comment on the article by Zentall, Wasserman, and Urcuioli allows me to thank them for their impressive body of work. Their pioneering efforts have influenced the careers of a large number of researchers who have been interested the analysis of stimulus control (me included). During my graduate training in Sidman's group at the Shriver Center, papers from these investigators were prominently featured within the curriculum and/or our lab meetings. In studying them, we learned a lot about designing stimulus control experiments and developing variations on basic matching-to-sample procedures that could be applied to enhance analyses of attending and remembering (e.g., Urcuioli & Nevin, 1975; Wasserman, 1976).

Pertinent to the work, there was ongoing debate back then about whether or not pigeons and other nonhumans could learn abstract concepts such as "same" vs. "different" via identity and/or oddity procedures (cf. Zentall & Hogan, 1976). Concerning this topic especially, we learned how difficult it was to design experiments that were capable of ruling out alternative explanations. Depending upon one's theoretical preferences and biases, one could usually find some way to either agree or disagree with the conclusions of researchers who asserted that nonhumans were capable of acquiring abstract concepts. For my part, I was convinced by Zentall, Edwards, Moore, and Hogan (1981) that pigeons could learn generalized "same" vs. "different" relations with at least some stimulus types. Whenever a colleague argued that pigeons could never learn such generalized relations, I referred to that paper and asked what procedural flaw(s) left him or her unconvinced. Although my colleagues typically did not change their opinions, I never had anyone provide me with a convincing argument that Zentall and colleagues (1981) were overreaching in their interpretation.

Over the last three decades, I have come to agree with the general thinking expressed in the target article: I think that the evidence has become overwhelming that pigeons and many other nonhumans are capable of stimulus–stimulus relational learning that cannot be explained by the basic processes defined by Ferster and Skinner (1957)—reinforcement, discrimination, response differentiation, and conditioned reinforcement. That is, the impressive transfer results contained in the body of work produced by Zentall, Wasserman, and Urcuioli are very likely *not* a result of mere perceptual discriminations masquerading as more advanced (i.e., arbitrary) relational performances. If one argues that such performances are masquerades, then one has the obligation to explain at the level of basic behavioral processes how such transfer effects could occur. Absent such an explanation, one should suspect ideological rigidity.

Convinced as I am that nonhumans are capable of learning arbitrary stimulus–stimulus relations, I am not yet convinced by the evidence in hand that nonhumans (pigeons and rats in particular) must ultimately and inevitably demonstrate first- and second-order stimulus equivalence relations. Demonstrations of such capabilities, of course, are required to support arguments that a given species exhibits behavioral processes relevant to the analysis of basic, subordinate, and superordinate categories. One can certainly make a plausible conceptual argument for the potential of pigeons to learn equivalence relations based on the totality of findings as Zentall and Urcuioli (1993) did some time ago. Moreover, the demonstrations of "backward association" (cf. Hogan & Zentall, 1977) by Frank and Wasserman (2005) and Urcuioli (2008) suggest that symmetrical stimulus–stimulus relations are within the range of pigeon capabilities. That said, I still have reservations about whether I will ever see pigeons and rats routinely demonstrating stimulus equivalence, thus supporting the proposition

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doi: 10.1002/jeab.59

that these species are useful for modeling fundamental aspects of human categorization.

I am not in the camp of those who think stimulus equivalence is a uniquely human phenomenon relating to development of language. For example, I was convinced that sea lions can exhibit true stimulus equivalence by the procedures and data of Schusterman and D. Kastak (1993) and the follow-ups (e.g., C. Kastak & Shusterman, 2002). I think also that the case for stimulus equivalence potential in great apes has been made fairly well, albeit somewhat indirectly by the extensive data stemming from efforts to teach these species rudimentary forms of language (cf. Sundberg, 1996).

As I survey the body of relevant work summarized by Zentall, Wasserman, and Urcuioli, I suggest recasting the basic question: Rather than asking "Are pigeons and rats capable of routinely learning stimulus equivalence relations?" I would ask "Can *we* become routinely capable of showing that pigeons and rats can learn equivalence relations?" Gratified as I was to see the citation to our conceptual work on stimulus control topography (SCT) coherence (McIlvane, Serna, Dube, & Stromer, 2000), its implications were considered only briefly. However, implicit in the discussion of the successive matching-to-sample procedures is a critical difference between contingency coherence theory (McIlvane & Dube, 2003) and Relational Frame Theory (RFT; Hayes, Barnes-Holmes, & Roche, 2001). Coherence theory posits that multiple exemplar training (MET) with symmetrical stimulus relations is not necessarily a requirement for emergent symmetry; it aligns with Sidman's (2000) proposition that the behavioral relations that define stimulus equivalence arise directly from the reinforcement contingency. MET in coherence theory is necessary only insofar as it is needed to align the experimenter's definition of the effective stimulus, response, and reinforcement with those that actually are the effective elements of the contingency for the subject. If the experimenter's procedures are designed sufficiently well to encourage coherence, then MET is unnecessary. By contrast, emergent relations in RFT are a direct product of MET on instances of those relations in the course of verbal interactions.

I believe that the procedures of Frank and Wasserman (2005) and Urcuioli (2008) yielded behavior consistent with symmetry because their procedures had features that encouraged contingency coherence (e.g., assuring successive discrimination among the stimuli). To advance the discussion, I will relay a personal experience with Dr. Urcuioli that will help explain the major point that I want to make in this commentary. In a colloquium at the Shriver Center some years ago, he presented the data that was reported in his 2008 paper. In the follow-up, I pointed out the many good features of his procedures and further suggested some procedural omissions that I thought might have resulted in the variability of outcomes across animals (i.e., there were several negative symmetry results among the positive ones in his critical Experiment 3). He made a colorful but totally on-point response that reduced to the following: "McIlvane, you might be right in your suggestions—but the procedures that you are describing would be a logistical nightmare with pigeons."

Dr. Urcuioli's response was entirely appropriate and correct in my opinion. At the time, I had come to refer to a set of procedures that I called "my $100,000 experiment" to demonstrate reliable equivalence relations in nonhumans. That name came from the per-subject budget that I estimated would be needed. Why such a large budget? The estimate was based on the need to give individual animals many hundreds of training and probe sessions. That number would be needed to assess and teach (as necessary) all of the requisite performances that coherence theory posits would be needed to assure positive results across the board. But who would finance an expensive pilot that had no guarantee of success?

I believe my time course and $100,000-per-subject estimates are not as unrealistic as they might sound. The training in Vaughan's (1988) study of functional class formation in pigeons was protracted and appeared to take more than one year to complete. Moreover, Schusterman and Kastak and the Rumbaughs conducted long-term training studies with sea lions and chimpanzees, respectively. Their work surely cost several (perhaps many) times the sum-per-subject figure that I estimate; it was made possible by large grants from the U. S. National Institutes of Health, the Department of Defense, and other generous funding sources.

With my colleagues Olavo Galvão, Ana Leda Brino, Romariz Barros, and others at the Universidade Federal do Pará (Brazil), I have been trying to assess whether we had the procedural maturity to attempt a version of the $100,000 experiment with a capuchin monkey. Regrettably, I do not think we are there yet. Although we can now establish generalized identity matching of abstract forms reliably (e.g., Brino et al., in press) with that species, reliable symmetry has proven elusive. However, this program has taught us many lessons about stimulus control—some of which informed my recent contribution to the *APA Handbook of Behavior Analysis* (2013)—that might position us ultimately to meet the challenges.

Retuning to Zentall, Wasserman, and Urcuioli and the implications of their work, we should examine whether the demonstrations of emergent symmetry without MET really disconfirm predictions of RFT. Critical as I have been of aspects of RFT and its applications (e.g., McIlvane, 2003), adherents have bases for questioning whether these demonstrations are as compelling as they might seem. RFT-oriented critics will certainly raise issues that recall similar ones posed decades ago concerning reports of generalized identity/oddity in pigeons:

Issue 1: Successful symmetry demonstrations have been small in number and restricted to one particular type of successive matching-to-sample preparation. Even within that particular preparation, intersubject variability has been substantial.

Issue 2. The behavior exhibited in these demonstrations has not much resembled the behavior of humans (or even sea lions) when they exhibit symmetry. The work with pigeons requires special procedures (e.g., successive matching, embedded identity MTS trials, etc.) and acceptance of relative response ratios rather than high accuracy as indicators of symmetry. By contrast, one can typically demonstrate emergent symmetry in humans without using special procedures or the need to accept test performances that include many inconsistent responses.

Considering both issues, RFT adherents can argue that emergent symmetry shown by pigeons and that shown by humans is fundamentally different in character. One could defend an assertion that behavioral processes studied in human equivalence research are not the same as those studied by Frank and Wasserman (2005) and Urcuioli (2008). While such arguments may be plausible, they will not be compelling without explaining how other well-defined behavioral processes could lead to the behavior observed. In this case also, I have not yet heard any such alternative accounts.

Considering the arguments put forward in the present article, I think that the authors do make a reasonably good case for the probable continuity across species of behavioral processes relating to emergent equivalence relations. Using legal language, I think their case is supported by "the preponderance of evidence." However, I do not think that the case has been proven "beyond a reasonable doubt." My opinion pertains not only to the findings and arguments presented in the present article, but also almost all of work relevant to the stimulus equivalence potential of nonhumans. Positive evidence to date has come from only a handful of subjects, key studies have virtually no independent replications, the inter- and intra-subject variability has not been adequately explained to my taste, and the level of behavioral technology needed to secure performances that meet the highest human standard does not yet exist.

I conclude by making a speculative prediction about the future course of empirical work in this general area. Zentall, Wasserman, and Urcuioli will be proven correct empirically in the essence of the arguments that they make in the present article—but not for many years. I am not talking only about the now-bleak prospects for obtaining funding to support basic behavioral research. For some years now, results of work with both nonhumans and nonverbal humans has begun to convince me that the matching-to-sample methodology that we all grew up with professionally will be shown to be unacceptably inefficient for answering certain types of questions. I believe the very frequent stimulus function reversals programmed in matching-to- sample may prove ultimately to unnecessarily challenge the neurology of common laboratory animals. In work with participants with severe intellectual disabilities (Serna, Dube, & McIlvane, 1997), we discovered long ago that children could readily sort identical stimuli well before they could exhibit matching-to-sample with the same items. My prediction (and hope) is that methodological research will yield more efficient, more effective procedures for promoting acquisition of multiperformance repertoires in common laboratory species. If so, we may well find that positive stimulus equivalence findings may become as reliable and replicable as those relating to the matching law.

Acknowledgement

Preparation of this commentary was supported by funding from the Commonwealth Medicine Division of the University of Massachusetts Medical School.

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Received: October 24, 2013
Final Acceptance: October 26, 2013